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REMARKS ON THE TEMPERATURE

OF SOME OF

THE INVERTEBRATES.

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The temperature of the invertebrates has been more or less carefully studied in Europe by Dutrochet,* Becquerel,† Valentin,‡ Newport,§ J. Davy,‖ and others. In the United States, however, I believe that no particular attention has been given to this subject; and although several of the foreign observers have made use of thermo-electric apparatus in their researches, yet, in this country, so far as I know, the investigations given in the present article are the first in which thermo-electricity has been systematically applied to the determination of the temperature of the animals in question.

In publishing these investigations, I am fully aware of their incompleteness, and my reason for having them printed is not that I consider them of so much value in themselves, but that I may in this way call the attention of those interested in phys-

* Ann. d'Hist. Nat., 2d series, Zoologie, t. xiii. † Traité de physique.

‡ Répert. de Anat. et de Physiol., 1839, t. iv.

§ Philosoph. Transactions, 1837.

‖ Researches, Physiological and Anatomical. Vol. 1st.

iological science to a subject that has been very much, if not wholly, neglected here, and especially to the application of thermo-electricity to the study of the phenomena of animal heat in general.

In determining the temperature of the invertebrates, we may use either the thermometer or one of the various forms of thermo-electric apparatus. The latter are, however, always to be preferred, being far more delicate, and likewise more convenient than the former. But if good thermo-electric apparatus and a sensitive galvanometer can not be obtained, we may, if the animal be sufficiently large, take its temperature by introducing the bulb of a very delicate thermometer into its interior, either through the natural openings or through an incision made for the purpose, the latter of the two methods being the one usually adopted by J. Davy.* If the animal be too small to admit of the introduction of the thermometer in either of the above ways, Mr. Newport's plan† may be pursued, which was to apply the external surface of the animal (in his experiments, an insect) to the bulb of the thermometer by means of a pair of pincers. These latter should be of sufficient length to prevent the radiation of heat from the hands from affecting either the animal or the thermometer, and the handles, if metallic, should be wrapped with some non-conducting material, in order to guard against the possible transmission of warmth by conduction. To avoid the effects of radiation, not merely from the person of the observer, but also from surrounding objects generally, Mr. Newport enveloped both insect and bulb with wool.

There is, however, an objection to this last procedure. The air entangled in the wool, not being changed with sufficient rapidity, becomes heated by the insect, and in turn reacts upon the latter, and the result is, that the animal exhibits a degree of heat which it could not, under ordinary circumstances, maintain when freely exposed to the atmosphere. This objection also applies to those cases in which the animal is examined in a glass tube or other vessel of small capacity.

There is also another objection to Newport's method. All investigations made by placing the external surface of the

* Op. cit., p. 195. † Op. cit.

animal in contact with the thermometer are faulty, for the very obvious reason that we do not in this way obtain the temperature of those portions of the organism in which the production of heat is greatest, the external surfaces of all animals being, as a rule, considerably cooler than their interiors.

Experiments conducted in the manner of Mr. Newport can, therefore, simply prove the existence of a heat-producing power, but can not give us the proper temperature of an animal.

If, instead of the thermometer, we use thermo-electric apparatus, we have our choice of several different instruments.

Nobili and Melloni* employed the thermo-electric pile, and by means of it first detected the presence of heat in insects. For this purpose they fitted to the faces of the pile small chambers of brass, in one of which the animal was placed, in the focus of a little reflector. The heat radiated from the animal fell upon the face of the pile, causing a deflection of the needle of the galvanometer, proportional, of course, to the difference in temperature of the two faces.† This method is manifestly objectionable on the same grounds as that of Newport, inasmuch as the animal is confined in a small volume of air, and the instrument does not obtain the temperature of the interior organs.

Still the pile is a very useful instrument for simply proving the existence of an independent source of heat in the inferior animals, when all other means have failed. Although the instruments to be presently described possess the advantage that they can be introduced into the interior of the organism, yet the pile is so much more delicate that it is capable of indicating differences of temperature which the other instruments, in spite of their contact with warmer organs, are unable to detect.

Instead of following the plan of Nobili and Melloni, I have generally secured the animal (always an insect) directly to the face of the pile, by ligatures, or by strips of adhesive plaster. In doing this great care must be taken to prevent the ligatures

* Treatise on Electricity by A. De La Rive. Vol. ii., part v., p. 571. Also, De la Chaleur produite par les etres vivants, par J. Gavarett, p. 117.

† This is not strictly true. The deflection of the needle is exactly proportional to the force of the current only up to a certain number of degrees, which has to be determined for each instrument.

or plaster from touching the antimony and bismuth bars, and also to prevent any of the fluids of the insect from coming in contact with these bars; in the case of grasshoppers this latter precaution is especially necessary.

Becquerel and Dutrochet invented instruments much better adapted to the determination of animal temperature than the pile, excepting in extraordinary cases.

Becquerel* employed two needles precisely similar, each about the twentieth of an inch in diameter, and composed half of steel and half of copper, one extremity being steel and the other copper. The centre of this compound needle consisted, therefore, of a soldered junction of two dissimilar metals, and consequently formed a thermo-electric battery.

One of these needles was pushed through the body of the animal under examination, until the junction reached the desired depth; its steel point was then connected with that of the other needle by means of a steel or iron wire—this second needle being left exposed to the air, merely taking the precaution to protect its junction from disturbing influences by means of a roll of dry paper. Lastly, the two copper extremities were connected with the galvanometer, and the needle of this latter instrument indicated by its deflection the difference in temperature of the two junctions.

These needles of Becquerel, although often serviceable, are yet, in many respects, inconvenient.

In the first place, we are always obliged to push the steel point entirely through the part the temperature of which is to be taken, in order that it may be connected with the other needle, and this is frequently a troublesome thing to do.

Their are other inconveniences attendant upon their use, that can not well be appreciated, except by actual experience.

The apparatus of Dutrochet†, like that of Becquerel, had its source of electricity in a copper and steel junction, but this junction formed a sharp point, which could be inserted into the body of the animal. This apparatus is, in many respects, preferable to that of Becquerel.

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* Op. cit., t. iv., p. 51, and Treatise on Electricity by A. De La Rive. Vol. ii., part v., p. 579.

* Op. cit., t. xiii., p. 5.

After a long and careful trial of the instruments just described, I devised a substitute for both.

In the first place, I have substituted German silver and brass for copper and steel. The iron or steel of the other instruments is apt to rust, particularly when exposed to an atmosphere saturated with aqueous vapor, as it often is in experiments upon the invertebrates. The metals are in the form of thin plates, three-sixteenths of an inch in width, and two and a half inches in length, to the place where they meet. They are separated from each other by a piece of bone rubber, an eighth of an inch in width at the upper extremities of the plates, but gradually narrowing down to the junction, at which point the two metals are welded together, so as to form either a sharp or blunt end, according to the particular purpose for which the instrument is intended. The insulating rubber runs up for the distance of an inch and a half or two inches above the commencement of the plates, forming a rounded handle, by which the instrument can be conveniently grasped.

The two brass plates are connected (for of course there is, as in Becquerel's needles, a pair of these instruments) by a wire, and the two silver plates connected each with a pole-cup of the galvanometer. Or, if we choose, we can connect the silver plates with each other, and the brass ones with the galvanometer, merely bearing in mind that the current is always from the silver to the brass through the junction, and consequently from the brass to the silver through the coil of the galvanometer.

This apparatus is not only more convenient than those of Becquerel and Dutrochet, but also much more delicate.

Whatever kind of thermo-electric apparatus we use, it is necessary to exercise the greatest care, for, as a rule, the evolution of heat in the invertebrate is feeble, and it is only by sedulously avoiding all sources of error that reliable results can be obtained.

No one should attempt to use thermo-electric apparatus without a thorough knowledge of the principal facts of thermo-electricity and of electro-magnetism. If this knowledge be not possessed, the indications of the galvanometer will be full as often interpreted wrongly as rightly. But even after a

theoretical acquaintance with the subject, it requires considerable practical experience to use the apparatus successfully in the more delicate experiments.

We must always bear in mind that we have not merely two sources of electricity in our circuit, viz., the two junctions, but that *every* deviation from homogeneity constitutes a battery. Every twist in our wires, every soldering, and finally the very brass pole-cups of the galvanometer, all may give rise to electric currents. In some of the very delicate investigations, the junction of the copper wires with the pole-cups of the galvanometer will, unless we be upon the watch, mislead us. It almost always happens, when a very sensitive galvanometer is used, that, at the moment the circuit is closed by connecting the copper wires with the galvanometer, the needle of the latter is deflected to a slight degree by the warmth of the hands communicated to the junction of the copper and brass, the current passing, in this case, from the brass to the copper through the junction.

Many pages could be written on the modes of applying thermo-electrity to the study of animal temperature, and on the errors to be guarded against; but the limits of this article forbid my devoting any more space to this part of the subject.

Having thus glanced at the different methods and means of investigation, let us next consider the particular experiments performed by myself.

Of the three great classes into which the invertebrates are divided, representatives of two, viz., the mollusca and the articulata, have been examined by me. Of the temperature of the radiata, I have no experimental knowledge; indeed, about all that is known of the temperature of these animals is derived from the experiments of Valentin solely.

Commencing with the mollusks, the temperature of the clam has been carefully studied by me. The mode of procedure was as follows:

My thermo-electric instruments (those with blunt junctions were used) were secured each by a brass claw, lined with cork, and fitted to an upright metallic rod in such a manner as to admit of both vertical and horizontal movement.

The instruments were held in the claws by their handles, in

a vertical position, with the junctions down. The animal was first examined in the air. For this purpose it was wiped as dry as possible, and then let alone for half an hour or more. It was then seized by means of a very long pair of forceps, and held directly under one of the the thermo-electric junctions. It was next forced to open its shell, when the claw holding the thermo-electric instrument was moved quickly down the upright rod, until the junction was fairly in the interior of the animal. As the clam invariably closed its shell tightly upon the instrument the moment it was introduced, the latter was held firmly in position. The other extremity of the animal rested upon a plate of glass. The second thermo-electric instrument had its junction protected by a roll of paper or by a piece of cork.

After waiting for a short time, in order to allow all effects produced by the hands to pass off, the connections were made with the galvanometer.

Operating in this way, I found that the temperature of the clam was invariably lower than that of the air, by a half or a quarter of a degree of F.

This was not surprising, considering, on the one hand, the naturally feeble production of heat by the animal, and on the other hand, the very copious evaporation that would necessarily take place from so moist a body, even after having been freed as much as possible from moisture.

In the next place, the temperature of the clam compared with that of the water was obtained, the animal being in the air.

To do this, the animal was quickly removed from the water, and the thermo-electric apparatus applied as in the former case, with the exception that the second junction was lowered into the water.

The result was, that the temperature of the clam was, in every instance, found to be lower than that of the water by half a degree of F., or even more; the influence of evaporation being, in this case, even greater than in the preceding, inasmuch as the clam was dripping wet. Finally, the temperature of the clam, compared with the water, while the animal was actually immersed in the latter, was taken.

Now, it is proper to remark, that although by taking the

temperature of an animal in water we put a stop to evaporation, yet if the volume of water be at all considerable, its high specific heat and better power of conduction render both its cooling and heating properties very much superior to those of dry air of the same temperature. Thus the specific heat of dry air is, according to Regnault, for equal weights, only 0.2375 compared with water as 1.000.*

This accounts for the fact, that Valentin found the temperature of marine animals to be sometimes lower in the water than in the air.†

Of course the greater the mass of water the greater will be its power of refrigeration, and consequently we can reduce this power to its minimum by making use of very small volumes of the liquid.

A glass vessel, but little larger than the clam itself, was therefore taken, and having been filled with water of the temperature of the air, the clam was immersed in it, and left untouched for several hours; at the end of this time the animal was seized with the forceps, and lifted only so far above the surface of the water as sufficed for the proper introduction of the thermo-electric instrument. This done, the animal was again submerged, with the junction still in its body, and at the same time the second junction was lowered into the water.

When all due precautions were adopted, it was generally found that the clam was warmer than the water, this excess never amounting, however, to more than a quarter of a degree of F., and being usually less than this.

During these experiments, as well as during those previously related, the temperature of the air and of the water ranged between 75 and 95 degrees of F.

It may appear, at first sight, that this mode of operating is open to the first of the two objections brought against Mr. Newport's method. It may seem that the clam is examined under circumstances more favorable to the maintenance of warmth than those under which it ordinarily exists, and that therefore, we can not by this method obtain the proper temperature of the animal. Such is not the case, however, for in its

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* Miller's Chemistry—Chem. Physics, p. 237. † Op. cit., p. 259.

natural state the clam lies imbedded in mud, and is, if any thing, protected in greater degree from loss of heat than in the experiment we have been considering.

There is another point of interest to be considered in this connection, and that is, that it is not necessary to find in a living animal a temperature superior to that of the air (provided the latter be not saturated with aqueous vapor) in order to prove the existence of an independent source of heat; for all animals are moist bodies, from whose surfaces a constant evaporation is taking place, and this evaporation tends to keep their temperature below that of the air. A moist, inorganic mass would, under like circumstances, cool down a little below the air, and therefore when we find an animal maintaining, in spite of the depressing influence of evaporation, a temperature equal to that of the atmosphere, we have proof of the existence of a heat-producing power, this proof being more or less conclusive according to the degree of saturation of the air.

I have examined the temperature of several other mollusks, but as the results have not been very satisfactory, I have not considered them worthy of publication, until the experiments from which they were derived shall have been repeated with more care and on a larger scale.

Passing next to the second great division of the invertebrates, viz., the articulates, we will consider in turn each of its subdivisions. First, the annelida.

Of this subdivision, the earthworm and leech have been examined by several observers, and my own experiments have also been made upon the same animals.

Hunter* introduced the bulb of a thermometer into a knot of earthworms, and found them to be a degree and a half or two degrees F. warmer than the air. He also found that the ordinary medicinal leech preserved a temperature one or two degrees above that of the air. Dr. J. Davy† took the temperature of two kinds of leeches in Ceylon, and found it to be the same as that of air. Dutrochet‡ and Berthold obtained results similar to those of Davy.

These few cases are about all that are recorded of the temperature of the annelida.

* Carpenter's Comp. Phys., p. 453. † Op. cit., p. 195. ‡ Op. cit.

In the experiments performed on the earthworm and on the leech, the same apparatus was employed, and the same general plan pursued as in the case of the mollusks, with the exception, however, that, as a rule, a saturated atmosphere was resorted to, instead of water, as a means of suspending evaporation.

The temperature of these animals varied very considerably. In many instances it was half a degree of F. or more above that of the atmosphere. In other instances the temperature was the same as that of the atmosphere, and again in some cases it was cooler than the air by a quarter of a degree of F. or more. In all these latter cases, the suspension of evaporation was invariably followed by a rise of temperature, which generally continued until the animal was warmer than the air by a quarter or half a degree of F. As a general thing, the leeches were a little superior in temperature to the earthworms.

Concerning the temperature of the second subdivision, the crustacea, but little is known. Valentin has recorded in four specimens of the crab the following temperatures above that of the surrounding medium: 0.54°, 1.08°, 1.62°, 0.18°, F.

Davy found the temperature of a large cray fish to be 79° F. when the air was 80° F., and that of a crab to be 72° F. when the air was at 72° F. likewise.

I have studied the temperature of two members of the crustacea, viz., the lobster and the shrimp.

The temperature of both of these animals is, so far as my own experience goes, invariably below that of the air or water by half a degree of F., or even more. Moreover, although every precaution was adopted, I never succeeded in obtaining positive proof of a heat-producing power in these animals. On stopping evaporation they warmed up somewhat, but never in any case attained a higher temperature than that of the surrounding medium.

There can be no doubt that every living organism, whether animal or vegetable, produces a certain amount of heat. These crustaceans must not, therefore, be considered as wholly destitute of a heat-producing power, but merely as cases in which this power is at its minimum, and consequently extremely difficult of detection.

I have not been able to find any recorded observations on

the temperature of the third subdivision, the arachnida; but from the few experiments that I have made upon spiders, I am inclined to consider their heat-producing powers as quite active; but more investigations are needed to establish this point.

Lastly we come to the insects, which, as a class, have had their temperatures more carefully studied than any other of the invertebrates.

As the results obtained by myself have been in the main confirmatory of those of previous observers, I will merely mention a few points that appear to be of especial interest.

The same instruments were used in these experiments as in those previously related, and in every case both insect and thermo-electric apparatus were covered with a bell jar.

I am inclined to think that, as a rule, the temperatures of the insects are best studied when the thermometer is below 70° F., rather than when it is above that point, and the reason is as follows:

We know that in the use of thermo-electric apparatus, the degree of deflection of the galvanometer needle is proportional to the difference in temperature of the two junctions, and that no matter how great the degree of heat or cold may be, so long as both junctions are equally heated or chilled, no indication is afforded by the galvanometer.

Now if we place one of our junctions in the mouth of a man, exposed to an atmosphere of 40° F., we obtain a deflection of the needle proportional to the difference between the temperature of the mouth and that of the air, for the second junction which is exposed to the air of course acquires its temperature. One junction, therefore, is at 40° F., and the other at 99° F., a difference of 59°.

But suppose we wait till a day comes when the thermometer is at 90°.

The junction exposed to the air is now at 90° instead of at 40°, but the temperature of the mouth is still 99°, or at least has risen only to a very insignificant extent. Consequently the difference between our two junctions is now only 9°, instead of 59°, and the deflection of the needle is correspondingly less.

As the temperature of the air rises higher, the deflection of the needle grows less.

Finally, if we place the man in an oven heated to 130° or 140° F. temperature, that can be easily borne for a considerable length of time, we find that the galvanometer indicates that the man is very much cooler than the air.

Now it is true that the cold-blooded animals do not possess by any means that same power of maintaining a constant temperature as do the mammals and birds; still their temperatures can not be raised or depressed indefinitely, and there are consequently limits both ways at which they exhibit a certain resistance.

Suppose, for argument's sake, the higher of these two limits to be for a given class of insects about 90° F., the result would be that, as the temperature of the air approached this point, the body of the insect would gradually become cooler than the air, for the junction exposed to the air would have its temperature raised, while the insect would not warm up with equal rapidity. This case is parallel to that of a man exposed to a temperature gradually rising to 99° and above.

Suppose, on the other hand, the lower limit to be near 60° F., as the atmosphere cooled down to this point the insect would gradually become warmer than the air, for the junction out of its body would fall with the air, while the insect itself would not have its temperature depressed quite so rapidly. This case corresponds, therefore, to that of a man exposed to a temperature falling by degrees from 130° to 98°, or thereabouts.

We see, then, that the same insect examined, first, when the air is at 90° F., and second, when it is at 60° F., would, in the former instance, be cooler, and in the latter, warmer than the surrounding medium.

I was led to these conclusions from the fact that all the insects examined by me in the spring, from about the last of March to the middle of May, when the weather was comparatively cool, gave indications of a temperature decidedly higher than that of the air. But soon after hot weather set in, their temperatures, as a rule, conformed very closely with that of the air; and as the mercury rose still higher, the majority of them

were cooler than the atmosphere. A single series of observations will serve to show this change.

On a day towards the last of March, when the temperature of the air was 60° F., a large moth gave, with Becquerel's apparatus, a deflection of 40° galvanometric degrees* on the hot side.

In the early part of July, the air being at 75° F., an insect similar in all respects to the first, caused a deflection of only 3° on the hot side of the galvanometer.

During the first part of August, the air being at 84° F., another insect of the kind caused a deflection of 4° on the *cold side.*

A few days after this, a fourth insect was also cooler than the air, which had the same temperature as in the last instance, by six galvanometric degrees.

Similar results were obtained with other insects.

To verify these results, an attempt was made to examine the insects in atmospheres of different temperatures artificially prepared. Owing to unavoidable interruptions I did not succeed in carrying out my plans to the extent that I had hoped; but the few satisfactory results that were obtained were all in favor of the conclusions already given.

There is an important fact to be observed in this connection, viz., that in placing an animal in either a high or a low temperature, the extremely poor power of conduction of its tissues renders a change in its temperature slower than in the majority of inorganic bodies of equal size—of course this is also true of a dead animal.

When, therefore, we place an insect in a higher or lower temperature than that of the air, we must always allow a certain time to elapse before we make the connection with the galvanometer, in order that we may eliminate the above source of error.

With the approach of the cool weather of autumn my investigations were repeated, but unfortunately circumstances again prevented me from performing them upon a sufficiently extensive scale; still, so far as they went, the results were in the main confirmatory of those already given.

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* Not to be confounded with thermometric degrees.

This was particularly true in the case of the grasshopper, an insect whose temperature I have carefully studied.

Of upwards of a hundred individuals examined during the hot weather of July and August, not a single one had a temperature above that of the air, and the great majority had a temperature below that point. When placed in air saturated with aqueous vapor, a portion had their temperatures raised as high as that of the air; but another still larger portion fell short of that point. In two or three cases the suspension of evaporation was followed by a rise of temperature on the part of the insect above that of the air; but these cases were so extremely few in number that I am inclined to attribute them to errors in experimenting.

During the last of September and the first of October twelve of these insects were examined. The temperature of the air was 70° F. and below. In every instance the suspension of evaporation caused the temperature of the insect to rise above that of the atmosphere, and in eight cases out of the twelve the insect exhibited a temperature slightly above that of the air before evaporation was stopped. In the remaining four cases the insect had the same temperature as the atmosphere.

I have thus given a synopsis of the principal part of my experiments on the temperature of the invertebrates, and trust that in so doing I have at least shown how much remains to be studied in this branch of physiology.